rendered necessary." They are very pleasant reading, and should be of considerable service to naval officers, yachtsmen, and sportsmen who may visit South America. The commission of the *Ruby* in South American waters extended over three and a half years, and during that time the officers seem to have missed no opportunity of indulging their taste for sport. The game killed amounted to 13,349 head. What was killed was never wasted, for there were on board 250 persons to be fed. Although Admiral Kennedy has much to say that will be especially interesting to sportsmen, they are by no means the only class of readers to whom he appeals. He visited many districts which are well worthy of being described, and the impressions they produced upon him are invariably recorded in a fresh, simple, and straightforward style. The text is illustrated with copies of some pen-and-ink sketches by the author.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Cirques.

PERMIT me, while thanking Mr. I. Russell for his friendly criticism of my views as to the origin of *cirques* (p. 317), and for a copy of his memoir on the Mono Valley, in which his own are developed, to indicate the reasons why I remain unconvinced.

(1) The first part of his criticism appears to me to be wide of the mark. I never said that the three conditions, quoted by him, were *necessary* for the formation of *cirques*, only that they were "the most favourable." I have described two *cirques* in granite (*Geol. Mag.*, 1871, p. 535), and seen them in other crystalline rocks.

(2) I never said that other topographical features might not be associated with those dispositions of rocks, which are often found in *cirques*. I stated that for the production of *cirques*, not only certain materials, but also a particular machinery, were required. Lines of cliffs, ravines, alcoves, corries, are numerous in the Alps: *cirques* are comparatively rare.

(3) The instances described in my original paper (Quart. Journ. Geol. Soc., 1871, p. 312) were called *cirques* at that date, and, I believe, still retain the name. They are in all essential respects identical with Gavarnie and similar places in the Pyrenees, which are always called *cirques*. Hence, it appears to me, Mr. Russell cannot elude my arguments by proposing a new terminology. Moreover, to call these places alcoves, is to misuse the word. An alcove holds a bed, or screens two lovers in earnest conference from over-curious eyes—it is a small affair. The diameters of the Alpine *cirques* are measured by hundreds of yards; their walls by hundreds of feet. Mr. Russell's studies in the Appalachians (unless I misunderstand him through ignorance of the locality) appear to me to indicate no more than that (as I have repeatedly affirmed) no real demarcation can be drawn between *cirques*, corries, and bowl-like heads of valleys. Of each I have seen examples, little as well as big. The formation of an alcove, as described by him, is only a special case of an action, identical in kind (but different in degree and environment) with that by which the above-named are produced. As it seems to me, he proves, not that *cirques* and alcoves are genetically distinct, but that they are genetically identical. One stream will make an alcove, many a *cirque*.

(4) I have never denied that under certain conditions a basin (generally quite shallow) may be scooped out by ice-action on the floor of a corrie, but the peculiar "tooth-drawing" action of the base of the *névé*, postulated by Mr. Russell, appears to me only an hypothesis. I stated, when it was propounded by Mr. Helland, that I knew of no evidence in its favour, and much in opposition (*Geol. Mag.*, 1877, p. 273); I repeat the same now. Wherever I have seen the bottom of a corrie uncovered, the rock has been smooth and ice-worn, not rough with the sockets of extracted "plugs" of rock.

(5) As a *bergschrund* usually narrows in descending, the tension of the ice should be at a minimum at the base of the *névé*, and I do not believe that atmospheric cold is appreciably more potent there than at the bed of a glacier. Moreover, as the ice must move more slowly in the *névé* basin than in the glacier, the former standing to the latter in the relation of a pond to its effluent, the friction there should be least.

(6) Some of the Alpine *cirques* lie rather low, and are *not* in a position where glaciers would first appear, as is shown by the fact that they are now full 3000 feet below the snow-line. Perhaps, however, Mr. Russell supposes rock wrenched away to a depth of quite a thousand yards. If so, I have nothing to add to what I wrote in 1871 and 1877. All that I know of the Alps and of other mountains is opposed to any such notion.

(7) Above three at least of the *cirques*, described by me, the cliffs rose in inaccessible steeps to the very summit of the range. From the highest peak of the Diablerets one could almost throw a stone on to the floor of the Creux de Champ, a full mile below; the two *cirques* near the Surenen Pass are immediately under the crest of the range (as shown in my diagram). Yet *there are the streams* fed by snow-ledges, which, though comparatively narrow, keep them still at work. Doubtless, in some cases the recession of the walls may bring to an end the work of excavation. But even in Egypt, in a *cirque*-like glen beneath a narrow crest, Mr. Jukes-Browne found evidence of streamlets (*Geol. Mag.*, 1877, p. 477); and I may cite Herr J. Walther (*Abhand k. Sächs. Gesellsch.*, xvi. 345) for the occurrence of "amphitheater oder circusthäler" in that country, though he attributes more to the action of wind than I should venture to do.

I might continue, but it may suffice to say that the result of twenty years' experience has been increased confidence in the general accuracy of the views expressed by me in 1871. These, it appears to me, Mr. Russell combats only by attempting a distinction, which I believe to be non-existent, and devising a method of glacier-erosion, which I believe to be not only mechanically impossible, but also contrary to the facts of Nature. T. G. BONNEY.

Bedford College and the Gresham University.

YOU have given publicity to various communications on the subject of the Albert or Gresham University; may we therefore request that you will extend a like favour to us, and insert a brief statement from one of the Colleges for Women, deeply interested in everything affecting higher education in and for London?

The following are some of the grounds on which the Bedford College, London, opposes the Draft Charter:—

(1) That the Bedford College, London, for Women, provides a complete academical course in the two faculties of arts and science, and therefore feels that from the very outset it is entitled to be included in any proposed University for London, on an equal footing with University and King's Colleges, with due representation on the Council. For such immediate affiliation the Draft Charter does not make any provision.

(2) That should the Draft Charter become law, a University would be created which, while exercising only some of the functions of a Teaching University, would do this in such a manner as to effectually prevent the foundation of any Teaching University which should be capable of expanding to the ever-growing demands of the Metropolis.

(3) That while the Act of 1871 abolished all ecclesiastical tests in the Universities of Oxford, Cambridge, Durham, and all Colleges therein to such an extent that all denominational Colleges are separate from the Universities, this Draft Charter of the Albert University allows one of its component Colleges to impose ecclesiastical restrictions. The Council, therefore, in its petition to both Houses of Parliament claims that the Bedford College be made an original constituent College in any new University for London, with due representation in the governing body, and prays that the present Charter be not granted unless so amended as to be far more comprehensive, more adequate to the present and future needs of the Metropolis, and free from all ecclesiastical restrictions.

W. J. RUSSELL, Chairman of Council.
LUCY J. RUSSELL, Honorary Secretary.
Bedford College, London, February 19.

The Implications of Science.

CAN you allow me space to reply to Miss Jones's courteous letter (p. 366), which I shall have the more pleasure in doing as I hope to be able to clear up the points in dispute between us?

I think it is clear—Miss Jones at least admits so much—that

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some part of the meaning of a term is determined arbitrarily; either by public opinion for general use, or by the individual reasoner, if he wishes to use the term for some special purpose of his own. For this does not imply that there ever was a formal contract as to the meanings of terms. The fact that terms do not mean the same to people of different nations is enough to show that the determination of the meaning by each nation is, in part at least, essentially arbitrary. It is also, I think, clear that there is a part of the meaning of some terms at least which is not arbitrary, when once the arbitrary part has been settled. There is a slight ambiguity about my use of the word "definition," which, however, when once noted, need cause no confusion, as the context will always show in which sense I am using it. Commonly a "definition" denotes an assertion which determines the arbitrary part of the meaning of a term; but when I speak of "the definition" as opposed to "the import" of a term, I mean not this assertion itself, but that part of the meaning of the term which it determines. This explains how it is that I do not regard "the definition" of a term (as opposed to its import) as a *thing*. Even if it is clearly conceived in the mind it is only an abstract idea; but the point is that it is not necessary that it should be conceived at all. Thus it is not necessary that a definition by connotation should be "of something," if that means that the term should have denotation. Even if the term has any denotation, *i.e.* if the mind grasps it as denoting a thing or idea, this denotation must, in a symbolic argument, be regarded as merely "accidental clothing."

Just as some part of the meaning of a term is arbitrary and some part not, so in any system of logic some part of the system is arbitrary and some part not. Now, either the denotation or the connotation of a term may be laid down arbitrarily, but *the connection between the two is not arbitrary*. One cannot arbitrarily lay it down that "such a thing possesses such attributes." If, therefore, a definition is to be an arbitrary assertion, it must only lay down either the denotation or the connotation of a term, and not parts of each. It would, of course, be logically permissible to use the word "definition" in a different sense, but then definitions would not be arbitrary assertions. This is exactly what is done if the assertion "Two straight lines cannot inclose a space" is called a definition by denotation; and therefore, in my system of logic, I cannot admit it to *be* a definition. This is one of the arbitrary features of my system. But it is not arbitrary when I say that in such assertions as "Nothing can both be and not be," and "Twice two is four," there is nothing absolute at all. The truth of these assertions is determined entirely by the arbitrary parts of the meanings of their terms. Miss Jones says: "If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* $(1+1+1)$ *means twice two* $(1+1)+(1+1)$?" Precisely; this is exactly the point. *There would be nothing at all to prevent it.* Boole actually did this very thing, as his law of indices in his mathematical analysis of logic. According to his law $a^{1+1+1} = a^{(1+1)+(1+1)}$.

There remain one or two minor points to be answered. As to induction, I think Miss Jones's view is substantially the same as mine. Of course it is possible to set out an inductive generalization in a syllogistic form as Miss Jones does; only the whole of the induction is then contained in the assumption of the major premiss—the syllogism itself is in no sense inductive.

If Miss Jones thinks the truth of the formula $(a+b)^2 = a^2+2ab+b^2$ is deduced by generalization from a single concrete instance, how does she explain the fact that sometimes $(a+b)^2=0$, as in Grassmann's "Aeussere Multiplication," and sometimes $(a+b)^2=a+b$, as in Boole's logic?

I see nothing to differ from in the paragraph about S and P; except perhaps that if I define such a term as "metal" by denotation I do *not* say "This and all other things like it in certain respects" are metal. That would be mixing up denotation and connotation in one definition. I say only "This, and this, and this, . . . are metal."

The explanation of the point about the mathematical truths is simply that I do not consider the assertion "Two straight lines cannot inclose a space" as a mathematical truth at all, if "straight line" is defined by denotation. I certainly believe it to be true, but its truth is not of the same nature as that of the assertion "Twice two is four," or even "*Cogito, ergo sum*"; it is neither a truism nor a necessary truth, in my senses of those terms, but can only be established by induction.

EDWARD T. DIXON.

Trinity College, Cambridge, February 19.

The Value of Useless Studies.

IT is rather surprising that Prof. Ayrton should indulge in covert sneers at Universities for devoting themselves to useless studies. It certainly ill becomes one whose life is bound up with electrical science, which is of such recent growth that nobody can pretend to forget how it owes its origin to those who studied it while useless. If Universities do not study useless subjects, who will? Once a subject becomes useful, it may very well be left to schools and technical colleges, to patent-mongers, and the trade. Mr. Bury is, on the other hand, mistaken in two respects. That a subject is useless is hardly worth considering as a recommendation for its being made *compulsory* on students. There are too many useless subjects for that. The great objection to compulsory Greek is that it is the principal stumbling-stone in the way of any literature being studied by ordinary University students. The Bible produced very little effect until it was read in translations; and the danger of a pagan revival, if ancient literature were studied without the obstruction of difficult languages, is the best reason for insisting on these languages in a Christian University. The second mistake of Mr. Bury is that it is any part of the business of a Universtiy to *teach*. Universities should certainly give facilities for students to *learn*. It is the business of the students to learn. If they are so ill prepared that they have not acquired the art of learning, they should go to a college or school or private teachers, and get taught; for teaching is the business of these institutions and persons. The business of Universities is to advance culture and knowledge, and to afford students an opportunity of learning how to do this. Prof. Ayrton, by omission rather than by commission, seems utterly unable to appreciate the value of literature for its own sake. How can all this fierce toil he extols so justly advance a lot of savages?

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, February 13.

The Nickel Heat Engine.

IN trying Mr. Smith's experiment on the rotation of nickel (NATURE, January 28, p. 294), I find my disk has a more complex action.

It will be remembered that the nickel is mounted on a vertical axis, and has the poles of an electro-magnet put near two points, A and B, 90° apart. Let C be any point in the larger arc, and suppose heat is applied to A.

I find that at first the disk moves in the direction ACB with a hesitating, uneven motion; it revolves several times, and almost develops into a steady rotation, when it beats back, oscillates for a while, and goes off with a brisk even spin in the direction BCA. After about two minutes this fails, and a new start is attempted the opposite way, but there is little more movement, until time has been given for cooling.

It makes no difference at which pole the heat is applied. The disk is 50 mm. diameter, rather less than 1 mm. thick, and is covered with lamp-black.

W. B. CROFT.

Winchester College, February 12.

THE UNIVERSITY OF LONDON.

PROBABLY before this letter is in type the Charter of the Albert, or, as it is now to be called, the Gresham, University, will have been presented to Parliament. What will be its ultimate fate remains to be seen. But I have sat through a long night's debate in the House of Commons to see a Government turned out in the small hours of the morning on a University question. I do not suppose that the proposed Charter will raise an issue of that importance. But I think that the Government may not be unmindful of past history, and, in what is probably the last session of a moribund Parliament, may not be willing to push very vigorously what is perhaps the crudest scheme of University organization which has ever been proposed in this country.

Before Parliament takes so serious a step as founding a new educational institution, the question may fairly be asked, On what ground is it necessary? Everyone knows that we have a University in London which is a State institution; and one may not unreasonably inquire,

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